

quickly follow, and we may thus hope to acquire, before long, a complete knowledge of one of the most wonderful florae and faunas of the world's surface.

P. L. S.

THE MOTION OF PROJECTILES

A Mathematical Treatise on the Motion of Projectiles, founded chiefly on the results of Experiments made with the author's Chronograph. By Francis Bashforth, B.D., Professor of applied Mathematics to the advanced class of Royal Artillery Officers, Woolwich, and late Fellow of St. John's College, Cambridge. (London: Asher and Co., 1873.)

WE are told in the Preface to this work that "the consideration of the motion of a projectile naturally divides itself into three parts—first, its motion in the bore of the gun; second, its motion through the air; and third, its motion during its penetration into a solid substance." The author directs his attention chiefly to the second of these parts. Galileo was the first person who determined with anything like accuracy the motion of a solid body moving through space under the action of gravity. Treating the vertical and horizontal motions as perfectly independent (which of course is in accordance with Newton's laws of motion), he showed that a particle moved in a parabola. In this theoretical investigation gravity is supposed to be constant, and to act in parallel directions, while the effect of the resistance of the air is totally disregarded. The parabolic motion is approximately true for bodies whose velocities are small, but the greater the velocity of a projectile, the more does its path deviate from a parabola, and, in the present days of large guns and heavy charges, we can at once see the importance of solving with the greatest possible accuracy the problem of the motion of a projectile through the air, considering the air as a resisting medium materially affecting the motion of the shot. Newton solved the problem of the motion of a body through a medium whose resistance varies as the first power of the velocity, and John Bernoulli extended it to the case of resistance varying as any power of the velocity.

Experiments, however, show that the resistance cannot be regarded as varying as any single power of the velocity, though, within certain limits, the third power gives pretty accurate results.

Mr. Bashforth has applied himself to the task of throwing Bernoulli's solution into a practical shape, so that by means of copious tables, of which his book contains more than 100 pages, such problems as the following may be solved:—"The 16-pounder muzzle-loading gun fires an ogival-headed shot 16 lb. in weight, and 3.54 inches in diameter. If the angle of projection be 2° , and the initial velocity 1,358 feet per second, find the trajectory and time of flight." "A Rodman shot weighing 452 lb. is fired with an initial velocity of 1,400 feet per second, at a target 500 yards off, find the striking velocity."

Experiments were made by Robins and Rumford last century to ascertain the pressure of fired gunpowder and several persons have attacked the problem during the present century. General Mayevski attempted to

solve the problem by firing shot, into the back of which a rod was screwed, the rod running through an aperture in the breech of the gun, and carrying a knife edge which cut two thin wires at a given distance, the interval of time between the two breakages being measured as accurately as possible. Captain Rodman made use of the following arrangement:—A gun was mounted in a gun-pendulum, and a revolving cylinder was placed with its axis parallel to that of the gun. When the gun was fired, a tracing point on the gun drew a curve on the revolving cylinder, the shape of which curve determined the whole motion of the gun's recoil. Mr. Bashforth suggested that much greater exactness would be procured if the tracing-point were connected with the projectile. He managed to do this to some extent by firing a shot through a number of equi-distant vertical screens, made of very thin metal wires. By an ingenious arrangement, the time of the shot breaking a wire in each screen was registered by means of an electric current on a revolving cylinder, special care being taken that all the registrations should be made under the same circumstances, so as to eliminate what we might call the personal error on the different registrations. This gave the times of transit of the shot over the successive intervals between the screens: from them, the velocities at the different screens can be calculated with great exactness, and also the resistance of the air on the shot. Mr. Bashforth has made great numbers of experiments with shots of different shapes and sizes, fired with different charges of powder, and from them has with great labour calculated the tables above referred to, which are sufficient for the solution of the problems we have given above as examples of what Mr. Bashforth has been able to accomplish.

The work is one which is too mathematical to do full justice to in our columns, but we have no hesitation in recommending it to such artillerymen as are not unacquainted with mathematical analysis.

OUR BOOK SHELF

Half-hours with the Microscope. By E. Lankester, M.D. (Hardwicke.)

THIS excellent and well-known little work would scarcely require to have special attention now drawn to it, if it were not that the present edition contains an additional chapter, which adds much to its value as a text-book for amateurs. Until now the subject of polarised light has been omitted, and as the many beautiful and striking results which can be obtained by its employment are among the most important and attractive in the whole field of microscopy, any work on the subject in which it is omitted must be necessarily incomplete. The author, evidently feeling this, has added a "Half-hour with Polarised Light," which he has entrusted to the hand of Mr. F. Kitton, who, in the short space allowed him, has explained the theory of this rather intricate subject in a clear and popular manner, and has described some of the most striking of the phenomena exemplified by it, such as the appearance of the slides of iodo-sulphate of quinine, asparagine and sulphate of copper in gelatin, together with the methods for arriving at them. The addition of this chapter has made this work as complete as it is useful to the commencing microscopist.

Proceedings of the Belfast Natural History and Philosophical Society. (Belfast, 1873.)

WE welcome with pleasure the first number of the Belfast Society's Proceedings, which includes a number of papers

read during the session 1871-2, some of which are already known to our readers. We need only name the principal papers. There is, first, the Presidential Address of 1871, "On Motive Power," delivered by Mr. J. J. Murphy, who has also a short paper on "The Bernina Lakes;" then comes Prof. James Thomson's admirable paper, "Speculations on the Continuity of the Fluid State of Matter, and on Transitions between the Gaseous, the Liquid, and the Solid States." This is followed by two short papers, one by Dr. J. D. Everett on "The Reduction of Observations of Wet and Dry Bulb Thermometers," and another on "Recent Changes of Coast-level at Ballyholme Bay, Co. Down," by Mr. Robert Young, C.E., who has also an excellent paper on "The Duty of Preserving National Monuments." Mr. John Anderson contributes a paper on "The Geological Formation of County Down," the Rev. Dr. Macloskie a long paper on "The Silicified Wood of Lough Neagh;" and there are also one or two papers of antiquarian and social interest. Appended is an interesting obituary notice by the secretary, Mr. Taylor, of the late Mr. Robert Patterson, F.R.S., one of the founders of the Society, and who, amid the cares attendant on the carrying on of a large commercial establishment, managed to find time to prosecute to very good purpose the study of natural history, and even to write admirable zoological text-books, and take an active part in the promotion of science and of social progress. The first number is edited by Mr. Murphy and Dr. H. Burden, and we hope the Society will produce material enough to bring out an equally good number every year.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Dr. Huizinga's Experiments

IN a letter published in last week's NATURE, in which Dr. Bastian comments on a short paper read by me at Bradford on certain experiments of Dr. Huizinga, he challenges me to deal with his "main proposition," which is "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles."

In so far as relates to the subject of my communication, I have done so by showing that in the case of Huizinga's liquid Bacteria can be prevented from arising by heating the liquid to a temperature somewhat above boiling.

I hope that Dr. Bastian will allow me to decline to enter on the general question, and will believe that in doing so I am not insensible either to the difficulties of the subject, or to the value and importance of his own experimental investigations.

Oct. 13

J. BURDON SANDERSON

Experiments on the Development of Bacteria in Organic Infusions

THE correspondence in your journal on this subject (relating chiefly to the statements of Dr. Bastian) in which I took a part some six or seven months since—renders it necessary, in justice to myself, and I may add, in justice to the memory of my friend, Dr. Pöde, whose loss has prevented me from continuing a series of experiments on the nutrition of Bacteria, commenced in the spring—to give some account in your columns of experiments carried out by us, which demonstrate that Dr. Bastian's assertions as to infusions of turnip and turnip-cheese are devoid of foundation in fact. The paper in which our results are given in detail was sent in to the Royal Society at the end of last March and printed in May (Proceedings, No. 145). Since you are not able to afford space for the reproduction of that paper in full, I must beg to refer your readers, for details, to that publication of the Royal Society. Here I may be allowed to sketch briefly the results and their bearing on Dr. Bastian's statements. The following passage from that gentleman's "Beginnings of Life" (vol. i. p. 429) induced us to make

experiments similar to those mentioned in it, with the view of testing the correctness of his conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torula*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "in a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion (see Appendix C, pp. xxxiv.-xxxviii), I fully believe that in 999 cases out of 1,000, if not in every case, a positive result could be obtained."

The extract which follows is from a paper by Dr. Bastian in NATURE, vol. vii. p. 275, and is perhaps more remarkable than the preceding, because it is of later date and refers to a simple infusion of turnip.

"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are my own.

Dr. Pöde and I give in our paper the details of 53 experiments, of which 11 were made with hay-infusion, the rest with turnip- or turnip-and-cheese infusion. We had some trouble at first in ascertaining some of the conditions under which Dr. Bastian experimented—since he does not state them in his book. In the first place we ascertained through these columns the specific gravity of Dr. Bastian's turnip-infusion. We made a number of experiments after obtaining that information, which are recorded in our paper, and which invariably gave opposite results to those obtained by Dr. Bastian. At the beginning of this year we ascertained through Dr. Sanderson, in the columns of NATURE, that Dr. Bastian made use of two-ounce retorts; and that particles of cheese visible to the naked eye were present in his infusions at the time of boiling. Dr. Sanderson also stated that Dr. Bastian attached importance to the peeling of the turnips used. With this additional information we made further experiments, which tend to explain the failure of Dr. Bastian to keep his infusions free from Bacterian contamination.

There are four points which require attention in these experiments, and which were attended to in our series, but we must suppose were not attended to by Dr. Bastian.

In the first place the infusions were examined by the microscope at the time of sealing the tubes, as well as subsequently. What we sought to determine was whether a change had occurred in the infusion. Spherical and other particles besides dead Bacteria occur in freshly-boiled infusions, which might lead to erroneous conclusions when seen subsequently, if their previous existence had not been ascertained.

Secondly, we employed small tubes, five inches in length and of half-inch bore. It appeared to us not at all improbable, from the results of some experiments made by us with retorts such as Dr. Bastian used, that a boiling for five or ten minutes, before closure, of an ounce of liquid in a vessel of that peculiar shape, might sometimes give a development of Bacteria, owing to the protective effect of "spluttering," and the large mass to be guarded.

Thirdly, in the majority of cases—though we had no reason to doubt the efficiency (as proved by the results) of the boiling for five minutes in one of our small tubes, *ceteris paribus*—yet, to ensure thorough exposure of every part of the tube and liquid to the boiling temperature, we submerged many of our tubes in boiling water for a quarter or a half an hour after their closure. This method we finally adopted as the most certain to ensure the destruction of Bacterian contamination; it is essentially the same as that subsequently adopted by Dr. Burdon-Sanderson in the experiments described in a letter in NATURE, vol. viii. p. 141, the difference being that, "to make assurance doubly sure," Dr. Sanderson raises the temperature of the water in which his tubes